FEDERAL SECURITY AGENCY
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Communicable Disease Center
Enteric Bacteriology Laboratory
Chamblee, Georgia

December 14, 1949

Dr. Joshua Lederberg
Department of Genetics
The University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

Thank you very much for your letter of December 7. It was regretted that such a long delay occurred in forwarding a report on the E. coli cultures which you sent to the laboratory. I can only say that considering the condition of the cultures and our lack of O serum for Coli 1 that the delay was more or less unavoidable. It was rather surprising to me to hear that you had not manipulated these cultures. Their condition more or less approximated that of cultures which we have received for study and which had been exposed to radioactive substances. Incidentally, we have been able to observe little change in cultures of the latter sort except that they seem to undergo rough degeneration.

Our thoughts regarding phage typing of enteric organisms are not that it could be used to replace serologic typing. Like you, we feel that it may be of value in breaking serological types into epidemiological entities. Problems in establishing a laboratory such as the one here are so numerous that many of the subjects upon which we would like to do intensive work are as yet untouched. Phage studies on various species of enteric organisms are among those projects which await development.

The third paragraph of your letter needs some discussion, I believe. First, let me say that we do not consider S. pullorum and S. gallinarum members of the same type. We consider these two organisms as perfectly distinct forms and I agree with you that their nutritional requirements are quite different. Also their action in fowls is usually quite distinct.

I agree that the work of Hohn and Herrmann had much to recommend it. It is unfortunate that they went completely overboard in drawing the conclusions which they did. We were able to confirm many of their observations but we could not agree that the nutritional varieties were associated with any particular host. I am taking the liberty of forwarding an old bulletin in which some work which we did on varieties of S. typhi-murium is described. It may be that you already have a reprint. If so, you can give this one to some

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interested person. I believe this subject is capable of further development but we have not worked on it for some years.

Your remarks concerning the citrate medium are in entire accordance with our thoughts on this subject. Recently Dr. Ewing and I have talked at length about this matter. These discussions were provoked by a paper published recently by W. B. Christensen of urea agar fame. A reprint of this paper can be obtained by writing to Mr. Christensen at the following address: Weld County Health Department, Greeley, Colorado. Christensen has pointed out that if citrate is combined with a source of nitrogen which can be utilized by the organisms that citrate utilization divides the enteric bacteria in an entirely different manner than does Simmons medium. For instance, the Alkalescens and Dispar types no longer are citrate negative but utilize citrate whereas the true dysentery organisms do not. This seems to be a reflection of the close relationships existing between Alkalescens and Dispar and the Coliform group.

I am particularly anxious to discuss with you your frequent observance of bacteriophage in Salmonella cultures. So far as I know at present I will be at the next SAB meeting in Baltimore. I hope to see you there and to discuss these matters.

For the Officer in Charge, Bacteriology Branch

Sincerely yours,

P. R. Edwards

Philip R. Edwards, Ph. D.
Bacteriologist-in-Charge
Enteric Bacteriology Laboratory

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